be obvious even with this rough test, but it certainly would not be sufficiently marked in nine months. We may therefore take it as settled that there is absolutely no evidence at the present time for supposing that radio-tellurium possesses a more constant radio-activity than polonium. If only this point has been made clear this correspondence may be considered to have justified itself.

According to Prof. Marckwald the idea associated with the term polonium is an extremely variable and indeterminate one. It seems to me that this is to put a wrong valuation on the work of its discoverer. Madame Curie gave the name to the hypothetical constituent of the bismuth separated from Joachimsthal pitchblende which caused its radio-activity. The radio-activity in question is distinct from that of any known radio-active substance, for it comprises only the emission of the α or non-penetrating type of radiation. Moreover, it slowly decays with time, and diminishes to half the initial value in about a year. Madame Curie has always been careful to point out that she has not succeeded in separating polonium from bismuth, or in obtaining any spectroscopic or other more direct proof of its existence. The name polonium applies to the body *causing this particular kind of radio-activity*. Hundreds of workers, I suppose, have obtained from the Société de Produits Chimique de Paris specimens of polonium prepared by Madame Curie's method, and have satisfied themselves by their own observations as to the character of its radio-activity. Now Prof. Marckwald has never claimed that he has isolated his body radio-tellurium, although he has been more fortunate than Madame Curie in effecting its concentration. Hence the name radio-tellurium applies also to the hypothetical constituent causing the radio-activity rather than to the preparation itself. Many, no doubt, have obtained also specimens of radio-tellurium from the firm of Dr. Sthamer, of Hamburg, and have compared its properties with those of polonium.

The meaning applied by Prof. Marckwald to the word polonium may be illustrated by these sentences quoted from his letter. "Shortly after the discovery of polonium Giesel found that this substance quickly lost its radio-activity. . . ." "Giesel's polonium emitted α and β rays and lost its activity within a few weeks." "In this (Giesel's polonium) I have found traces of radio-tellurium, and I have shown that after the removal of the latter the remaining substance shows strong β and diminished α radiation."

The question at issue is therefore a very simple one. Is Prof. Marckwald justified in applying Madame Curie's name to Prof. Giesel's preparation? "Giesel's polonium," according to Prof. Marckwald's statement, is a mixture of two radio-active constituents:—(1) radio-tellurium, (2) a constituent giving β as well as α rays. The latter, since it can neither have been polonium nor radio-tellurium, need not be further considered in the present discussion. It *may* be something new and interesting, but, on the other hand, there is nothing to show that it was not merely a trace of radium present as an impurity. In either case it does not concern us, and two bodies only, Madame Curie's polonium and Prof. Marckwald's radio-tellurium, need be further considered. Both are obtained from the same variety of pitchblende, both are distinguished from all the other radio-elements by the fact that they only give α rays, and both possess at least a considerable fraction of their initial activity after the lapse of one year. Now Prof. Marckwald used the same[1] raw material as Madame Curie, namely, the bismuth extracted from the Joachimsthal pitchblende. Since he states that his method separated *all* the active constituent we may feel certain (1) that radio-tellurium must certainly contain polonium; (2) that as it gives no β rays it contains none other of the known radio-active elements; (3) that as the radio-active properties of the two preparations are indistinguishable the active constituent of Prof. Marckwald's preparation is the same as that of Madame Curie's preparation, and therefore by every recognised canon should be termed polonium.

Prof. Marckwald's work has shown that there are present on a maximum estimate 4 milligrams of the active constituent in two tons of pitchblende, or in 8 kilograms of the bismuth salt separated from it. Hence what possible bearing can such a small trace of substance have upon the analytical reactions of the relatively vast bulk of the raw material? In laying stress on these reactions he frequently seems to apply the term polonium to Madame Curie's preparation rather than to its radio-active constituent.

The same criticism might be applied to the following sentence, to be found in his most recent communication (*Berichte*, 1903, p. 2665). "Whether this Curie's polonium does not perhaps contain also some radio-tellurium is a question which must be left to the discoverers of polonium."

With regard to the view expressed that polonium is merely radio-active bismuth, or inducedly active bismuth, in support of which an opinion once expressed by Madame Curie is quoted, the answer, of course, is that Prof. Marckwald's own subsequent work has shown otherwise. By the experiment of depositing on a stick of pure bismuth the whole of the polonium present in a solution, he makes it evident that the latter cannot be bismuth. Those who are acquainted with the work of Rutherford in 1900 on "induced" activity know that the whole conception of radio-active induction has been built up on a simple misconception of the phenomena it is designed to explain. The conception had its origin in the belief that the *rays* from a radio-active substance could excite radio-activity in otherwise inactive matter, which was not in accordance with the facts known at the time it was put forward.

Giesel repeated the identical experiment of Prof. Marckwald with a solution of pure radium, and found that a stick of bismuth after immersion becomes permanently (?) active and then only emits α rays, and Prof. Marckwald, in spite of his own work, concludes that there exists with certainty an inducedly active bismuth giving only α rays, which might with accuracy be termed polonium. He, however, omitted to state that Giesel obtained the identical result if a stick of platinum or palladium were immersed in the radium solution. Hence it might be argued that there exist an inducedly active platinum and an inducedly active palladium, both of which might with accuracy be termed polonium. The alchemists considered that they had turned iron into copper by means of a solution of blue vitriol, until it was pointed out that the latter substance contains copper. It has never been shown that any of the effects of the so-called "radio-active induction" are really due to the conversion of an inactive element into radio-active matter. From the existing evidence to the contrary, it would seem more reasonable to suppose that they admit of a similar interpretation to that now adopted to explain the cuprification of iron.

FREDERICK SODDY.

[1] The point raised in the footnote to Prof. Marckwald's letter is, I take it, a side issue. He brings forward no evidence that the bismuth separated from the pitchblende by sulphuretted hydrogen (Curie) is different in its radio-active properties from that separated by himself as oxychloride, nor any reason for supposing that the active constituent in the two cases might be expected to be different. It is true that his bismuth contained a minute proportion of ordinary inactive tellurium, which was probably almost or quite absent in Madame Curie's preparation. This fact he made use of as the basis of his elegant method of concentrating the active constituent, and he seems to have at first confused the difference of behaviour of the two raw materials to differences in the chemical nature of the active constituents rather than to the fortuitous presence of a trace of tellurium. But his own later experiments (*Beri hte*, 1903, p. 2663) show that when the tellurium is removed from the solution his methods of precipitating the active constituent completely fail, but again work perfectly if a few tenths of a milligram of ordinary telluric acid in aqueous solution are added.

Dependence of the Ionisation, produced by Röntgen Rays, upon the Type of the Rays.

MR. EVE, in his letter in NATURE of March 10 (p. 436), shows that the relative amount of ionisation produced by Röntgen rays in different gases depends upon the "hardness" or penetrating power of the rays. I have lately been investigating this question of the dependence of the relative ionisation upon the type of rays, and an abstract of a preliminary paper on the subject appeared in a report of the proceedings of the Cambridge Philosophical Society in the number of NATURE issued on February 18 (p. 383). These experiments, along with later ones, show that the relative ionisation in different gases depends upon the type of rays used. I used a balance method, balancing the ionisation in each gas against that in air. The pressure of the gas in the Röntgen ray bulb was varied, thereby varying the "hardness" of the rays, and it was found that in the case of gases in which the ionisation is greater than in air the ionisation in these gases decreases relatively to that in air

as the rays become harder. This result is in agreement with that given by Mr. Eve. I find also that in hydrogen, in which the ionisation is much less than in air, the ionisation increases relatively to that in air with the increase of hardness of the rays.

The experiments are not quite completed yet, but it is hoped to publish a full account of them shortly.

R. K. McCLUNG.

Cavendish Laboratory, Cambridge, March 12.

Polarisation in Röntgen Rays.

IN a paper on secondary radiation from gases subject to X-rays (*Phil. Mag.* [6] v., p. 685, 1903), I described experiments which led to the conclusion that this radiation is due to what may be called a scattering of the primary X-rays by the corpuscles (or electrons) constituting the molecules of the gas. More recently I have found that from light solids which emit a secondary radiation differing little from the primary, the energy of this radiation follows accurately the same law as was found for gases, so that the energy of secondary radiation from gases or light solids situated in a beam of Röntgen radiation of definite intensity is proportional merely to the quantity of matter through which the radiation passes. Experimental evidence points to a similar conclusion even when metals which emit a secondary radiation differing enormously from the primary are used as radiators, though I have as yet only shown that the order of magnitude is the same in these cases. The conclusion as to the origin of this radiation is therefore equally applicable to light solids, and probably to the heavier metals.

As explained by Prof. J. J. Thomson ("Conduction of Electricity through Gases," p. 268), on the hypothesis that Röntgen rays consist of a succession of electromagnetic pulses in the ether, each ion in the medium has its motion accelerated by the intense electric fields in these pulses, and consequently is the origin of a secondary radiation, which is most intense in the direction perpendicular to that of acceleration of the ion, and vanishes in the direction of that acceleration. The direction of electric intensity at a point in a secondary pulse is perpendicular to the line joining this point and the origin of the pulse, and is in the plane passing through the direction of acceleration of the ion.

If, then, a secondary beam be studied, the direction of propagation of which is perpendicular to that of the primary, it will on this theory be plane polarised, the direction of electric intensity being parallel to the pulse front in the primary beam.

If the primary beam be plane polarised, then the secondary radiation from the charged corpuscles or electrons has a maximum intensity in a direction perpendicular to that of electric displacement in the primary beam, and zero intensity in the direction of electric displacement. Prof. Wilberforce first suggested to me the idea of producing a plane polarised beam by a secondary radiator, and of testing the polarisation by a tertiary radiator.

The secondary radiation from gases is, however, much too feeble to attempt the measurement of a tertiary. From solids I think it will be possible, and hope shortly to make experiments on this.

It occurred to me, however, that as Röntgen radiation is produced in a bulb by a directed stream of electrons, there is probably at the antikathode a greater acceleration along the line of propagation of the kathode rays than in a direction at right angles; consequently, if a beam of X-rays proceeding in a direction perpendicular to that of the kathode stream be studied, it should show greater electric intensity parallel to the stream than in a direction at right angles.

I therefore used such a beam as the primary radiation, and studied by means of an electroscope the intensity of secondary radiation proceeding from a sheet of paper in a direction perpendicular to that of propagation of the primary beam.

By turning the bulb round the axis of the primary beam studied, the intensity of this beam was not altered, but the intensity of the secondary beam was found to reach a maximum when the direction of the kathode stream was perpendicular to that of propagation of the secondary beam, and a minimum when these two were parallel.

In one series of experiments the intensity of secondary radiation in a direction perpendicular to that of the primary beam was compared with that in a direction making a small angle with the axis of the primary beam. The latter, according to theory, should not vary with the position of the X-ray bulb.

In a second series of experiments the intensity of secondary radiation in a direction perpendicular to the axis of the primary beam was compared with that of a small portion of the primary beam itself, when the bulb was in different positions.

Lastly, the intensity of secondary radiation was measured in two directions perpendicular to that of propagation of the primary radiation and perpendicular to each other, while the intensity of the primary beam was measured by a third electroscope.

The three methods gave similar results.

In the last case, as the bulb was turned round as described, one secondary beam reached a maximum of intensity when that at right angles attained a minimum. When the bulb was turned through a right angle the former produced a minimum of ionisation while the latter produced a maximum.

Two bulbs were used and the sizes of the apertures were varied, but the results were similar in all cases.

The variation of intensity of the secondary beam amounted to about 15 per cent. of its value, but this, of course, does not represent the true difference, as beams of considerable cross section were studied, consequently secondary rays making a considerable angle with the normal to the direction of propagation of the primary rays were admitted into the electroscope.

The experiments are being continued.

These results, however, are in agreement with the theory, and I think show conclusively that the X-radiation proceeding from a bulb is partially polarised.

CHARLES G. BARKLA.

University of Liverpool, March 10.

The British Government and Marine Biology.

IN a note in your issue of February 25 announcing the appointment of Mr. James Hornell, who, it is stated, acted as Prof. Herdman's assistant during the Ceylon pearl oyster investigation, to the post of marine biologist to the Government of Ceylon and inspector of the pearl banks, it is said that "the appointment is of interest as showing how in the recognition of science some of our colonies are in advance of the mother country. We have no 'marine biologist to the Government' here."

Now although the latter statement may be verbally accurate, it appears to me to be misleading, and one would seem to be justified in supposing that it has been made without full knowledge of the facts.

At the present time the British Government is committed to an expenditure of 42,000*l.*, to be spread over a period of three years, for the purpose of carrying out the British portion of the international fishery investigations, the programme of which, conceived in an eminently scientific spirit, has been drawn up by an international council comprising amongst its members some of the most distinguished of European marine biologists. In addition to this the Government has made for a number of years, and still continues to make, a grant of 1000*l.* a year to the Marine Biological Association, the declared object of which is the promotion of both scientific and economic marine biology; public money has been spent on scientific fishery investigations in both Scotland and Ireland, and the Government has quite recently appointed Dr. A. T. Masterman, a well known and capable marine biologist, to the post of inspector of fisheries.

To decline to acknowledge what is already being done is surely not the way to obtain increased support for scientific investigations in the future.

E. J. ALLEN.

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THE brief statement contained in the note was quite correct, and although it might be expanded and illustrated, it needs no qualification. We were well aware of all the facts stated by Dr. Allen.